of the mollusks are thinner than is usually the case with those of the littoral zone; and *Fredericella Duplessis*, which is the representative of *F. sultana*, has so far varied from the littoral form that it is never found attached to solid bodies, such as pebbles or fragments of coke, but invariably plunged in the soft ooze after the fashion of a Pennatula.

But of two species of Crustaceans—*Niphargus puteanus* var. *Forelii*, an Amphipod, and *Asellus Forelii*, an Isopod closely related to *A. cavaticus*—Prof. Forel maintains that they are descended, not directly from the allied species of the littoral zone, but from the species *N. puteanus* and *A. cavaticus* which inhabit the subterranean waters, and are commonly found in the wells of nearly the whole of Europe. There is no doubt that *A. Forelii* is closely related to *Asellus aquaticus*, nor that *Niphargus puteanus* is equally closely related to *Gammarus pulex*; the question is merely whether the forms at present inhabiting the abysses of the Léman and other lakes are like *Fredericella Duplessis* directly descended, or indirectly descended, from the littoral forms. It was to the first of these views that Prof. Forel formerly inclined: he now gives his support to the second. And mainly for the following reasons. The modifications which *Niphargus puteanus* and *N. Forelii* have undergone are in all important respects the same; they differ at present only in such unimportant points as the number and length of hairs, setæ, and spines. It is unlikely that precisely the same changes would occur under such very different surroundings as those presented by subterranean waters and the deep waters of a lake. Again, *N. Forelii* is not confined to the Léman, and it is improbable that exactly the same variations should have arisen in different localities. And thirdly, since maintaining a lacustrine origin for *N. Forelii* would compel us to admit that it had varied so far from *Gammarus pulex* since the Glacial period, we can by supposing it to have a subterranean origin allow it a far longer time in which to have undergone modification.

"It is more simple, it is more in conformity with facts to admit that the *N. Forelii* of our lake-bottoms is descended from the *N. puteanus* of the underground waters. That is the conclusion to which I adhere. And I extend this same conclusion to *Asellus Forelii*, and seek its origin also in the *A. cavaticus* of the underground waters."

An interesting illustration of the manner in which animals can adapt themselves to their surroundings is to be found in the species of Limnea and in the larvæ of Diptera (Chironomis) which abound in the Léman. In the littoral zone the Limneas, having a pulmonary sac, are air-breathers; in the deep water, without any change of structure, their breathing is aquatic—their pulmonary sac is filled with water. The case of the Dipterous larvæ is more remarkable. We are told that they swarm in the deep water, and that their respiratory apparatus, consisting of tracheæ, is, like the sac of the Limneas, filled with water instead of air. Larvæ abound, but pupæ are very rare, if not altogether absent, and perfect insects are never seen rising from the surface of the deeper parts of the lake; moreover larvæ of all sizes and ages are found on the bottom at the same season. It would appear from the observations of O. Grimm (*Mém. Acad. imp. St. Pet.*, xv. No. 8, 1870), of St. Petersburg, that these larvæ never attain the perfect stage, but are capable of reproduction by *pædogenesis*.

I have no more space; I can only allude to the discovery of two species of Acanthopus, whose nearest relatives are marine Cythærideæ; to *Plagiostoma Lemani*, also with marine relations; to the remarkable absence in the deep water of Anodon and of Spongilla, both of them so common in the shallows. Let me conclude in Dr. Forel's words:—

"Others may perhaps regret the absence of the strange things which they had expected to meet with in these strange regions. For my part I have had the intense happiness of being the first to penetrate them, I have endeavoured to explain to myself one by one the mysteries which unfolded themselves to my gaze, and I admire and enjoy their harmony and their simplicity above all. Nature is beautiful and great because she is harmonious everywhere and in everything."

G. H. WOLLASTON

THE CRETACEOUS AND TERTIARY FLORAS OF THE UNITED STATES

The Cretaceous and Tertiary Floras of the United States. By Leo Lesquereux. (U.S. Survey of the Territories under F. V. Hayden, Vol. VII.)

AFTER an interval of nearly ten years, Dr. Hayden presents us with further contributions, by Lesquereux, to the Cretaceous and Tertiary floras of the United States. Those principally illustrated are from the Dakota, Laramie, and Green River groups. The author frankly admits at the outset, p. 4, that "the determinations of the plants are still, and must be for a long time to come, unreliable to a certain degree." This admission must be kept in mind in pronouncing on the merits of the book.

The Dakota beds rest on Permian, and contain a Cretaceous fauna associated with a very rich dicotyledonous flora. No one now doubts their Cretaceous age, although they cannot be correlated exactly, bearing in mind the flora, with anything in Europe. It appears from the revision these fossil plants have undergone, that they are much less closely related to existing genera than was previously supposed. Under such circumstances it seems a pity that less compromising generic names were not substituted for those, such as Sassafras, Acer, Quercus, Hedera, &c., as done in the case of Populites. The known flora of Dakota now consists of 5 ferns, 6 Cycads, a dozen Coniferæ, most of them very unsatisfactory, and no less than 162 Dicotyledons, chiefly remarkable for the large number of handsome palmate leaves among them. One of the most interesting genera, because determined from fruits as well as leaves, is Platanus, a genus also common to our own Lower Eocene of Reading, and thus of a high antiquity. Magnolia is another genus adequately determined, but the remainder rest mainly, if not entirely, on the characters furnished by detached leaves. The vexed question of the age of the Laramie or Great Lignitic series of America is again discussed, and a table given of all its species compared with those of Europe, especially the Eocene of Sézanne in the Paris Basin. As a result

the author still holds to the opinion that the formation is an Eocene one. A larger part of the work is occupied with descriptions of the Green River plants, chiefly from Florissant, an incredibly rich locality. This is prefaced by a lucid description of the beds, which exceed 300 feet in thickness, by Mr. S. Scudder. They are principally volcanic ash accumulated in one or more old lake-basins. These and most of the other fossiliferous rocks are situated towards the top of the Green River group, which is reckoned to be 2000 feet thick. The flora contains 228 species, of which 152 are from Florissant, and is referred by Lesquereux to the Oligocene. It was originally thought by him to be Miocene, but the detailed comparisons he has made between it and that described by Saporta from Aix, in Provence, prove that he is justified in putting its age further back. Indeed it bears a marvellous resemblance to that of Bournemouth, and had he been able to make comparisons he would perhaps have assigned it a still earlier date. It is a matter of the greatest interest to find in America a flora corresponding to those of Aix and Bournemouth, and not represented anywhere to the north. The last pages are occupied with descriptions of some new Miocene plants from various localities. The book is illustrated by fifty-nine coloured plates, and however we may differ as to the value of the determinations themselves, all will agree as to the great service rendered to science by the publication of such an important mass of data for future comparison. J. STARKIE GARDNER

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Coal-Dust Question

ON several occasions during the last five years, Sir Frederick Abel has referred to the history of the coal-dust question, and to my connection therewith; and as his views on this subject do not altogether correspond with mine, I desire, with your permission, to state in this place how much, and in what particulars, I differ from him.

In his address to the Society of Arts, delivered on the 17th of November last, Sir Frederick says: "Several well-known French mining engineers published, many years after Faraday and Lyell wrote, observations and experimental results as new, which were simply confirmatory of those philosophers' original statements and conclusions, and to some extent this was also the case in still more recent publications in this country by Galloway and Freire-Marreco."

Faraday and Lyell's statements and conclusions were to the following effect:—

1. Fire-damp is not the only fuel in an explosion.

2. The coal-dust is swept up by the blast and is partially burnt.

3. (Speaking of Haswell Colliery explosion)[1] "There is every reason to believe that much coal-gas was made from this dust in the very air itself of the mine, *by the flame of the fire-damp*, which raised and swept it along, and much of the carbon of this dust remained unburnt only for want of air."

That is to say, the flame of the fire-damp is extended, and the effect of the explosion is aggravated by the presence of the coal-dust.

[1] *Phil. Mag.*, 1845.

The Committee of the Coal Trade, who replied to Faraday and Lyell's report in a letter dated February 7, 1845, do not appear to have thought those authors' remarks about coal-dust of sufficient importance to be noticed at all. Nor do the authors themselves seem to have attached any particular importance to them save as a record of a curious physical and chemical fact; for after making them they immediately turned, like all their predecessors and most of their successors, to the contemplation of imaginary magazines of fire-damp as a means of accounting for the explosion. Moreover, both of them lived for many years afterwards, during which one great explosion occurred after another, and yet we do not find that either of them ever lifted so much as the tip of his little finger to point to coal-dust as the probable cause of the catastrophes.

In 1855, M. du Souich, Ingénieur des Mines, said,[1] "A sort of crust of light coke, which could be gathered from the timber at various points, could only have originated from the coal-dust swept up in the working places and carried to a distance by the extremely violent air-current caused by the explosion. This dust being itself partially inflamed *could continue the effects of the fire-damp by carrying them further*" (*peut continuer les effets du grisou en les portant plus loin*).

In 1861,[2] M. du Souich and M. Estaunié again insisted on the same thing in similar terms.

In 1867,[3] M. du Souich again developed the same opinions.

In 1864,[4] Verpilleux de Reydellet, A. Burat, Poumairac Baretta and other engineers emitted opinions similar to the foregoing.

In 1875,[5] M. Vital wrote, "Extremely fine coal-dust is a cause of danger in dry working places in which shot-firing is carried on; in well-ventilated workings it may of itself alone give rise to accidents; in fiery workings it increases the chances of an explosion, and when an accident does occur *it aggravates the consequences of the fire-damp flame*" (*coup de feu*).

In 1875,[6] MM. Desbief and Chansselle gave a short historical *résumé* similar to that of M. Haton, and quoted an opinion of M. Verpilleux which appears to resemble my own, but has never, so far as I am aware, been put prominently forward by the author nor supported by experimental or other proof, namely: "M. Verpilleux, who attaches great importance (*importance capitale*) to coal-dust, was one of the first to call attention to it; comparing a fire-damp explosion to the detonation of a gun, he went so far as to say that the dust represents the powder and the firedamp the priming."

In March, 1875, Sir Frederick, then Professor, Abel, addressing an audience at the Royal Institution on the subject of "Accidental Explosions," referred to explosions in mines, and mentioned the researches that had been made by Mr. R. H. Scott and myself up to that date. He also speaks of dust explosions in flour mills, &c.; but, as showing the small importance he attached to anything that had been previously said or done in regard to coal-dust, it is remarkable that he does not even refer to its existence.

I had been investigating the subject of great colliery explosions since the year 1870, but had been unable to discover any explanation of their occurrence wholly satisfactory to myself. At the commencement of my work I had read all, or nearly all, the English literature connected with it then extant, and amongst other things the report and article of Faraday and Lyell on the Haswell Colliery explosion, and the reply of the Committee of the Coal Trade;[7] but, so little impression did Faraday and Lyell's remarks about coal-dust make upon me at the time, that I afterwards forgot I had read them, and was only reminded of the fact by seeing it recorded in an old note-book of my own some time after the publication of my first paper. I retained the impression, however, for in the paper[8] referred to I wrote: "The accounts of colliery explosions published in this country hardly ever allude to the existence of coal-dust; and when they do so, in one or two cases [it should have been one case only] it is for the purpose of suggesting that the gases disengaged from it by the heat of the fire-damp flame would no doubt be ignited and tend to increase the force of the explosion."

It did not for a moment occur to me that this, which is Faraday and Lyell's view, could ever be accepted as an explanation of the phenomenon I was trying to elucidate.

[1] "Rapport de M. Haton de la Goupillière: Des Moyens propres à prévenir les Explosions du Grisou," 1880.
[2] *Ibid.* [3] *Ibid.* [4] *Ibid*
[5] *Annales des Mines*, 1875.
[6] *Comptes rendus des Réunions mensuelles de Saint Etienne*, June, 1875.
[7] *Iron*, June 1, 1878. [8] *Proc. Roy. Soc.*, 1876.